THE FUNCTION OF SCIENTIFIC THEORY IN THE THOUGHT OF T. F. TORRANCE

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Abstract: *T. F. Torrance's interest in the nature of natural and theological science is well known, but his position relative to major contemporary philosophers and philosophical traditions is not. This paper surveys the positions advocated by several of the most important philosophers of science in the twentieth century and locates Torrance's views within that landscape. While few, if any, of the consequences of Torrance's understanding of scientific theory are unique, when taken as a whole they provide a compelling view of science that is rooted in distinctly Christian convictions.*

Introduction

T. F. Torrance's interest in the natural sciences, both in themselves as well as in their function as dialogue partners with Christian theology, is well known. However, Torrance was not particularly clear as to how his thoughts on the nature of science were either similar or dissimilar to the main views propagated during his academic career. Indeed, if one were to read Torrance's scientific and epistemological writings, one might be left with the idea that Torrance represents something like the consensus view among scientists and philosophers of science. While it is true that very little in Torrance's understanding of science is unique, it is clearly distinguishable from the most influential views put forward by major philosophers of science.

This paper intends to summarize the way various philosophers in the twentieth century understood the function of scientific theory and then to

show how Torrance's views are either similar or dissimilar from them.[1] It is not possible to understand Torrance's subtle and nuanced perspective unless one understands the major landmarks in the philosophical landscape at the time. Out of concerns for space, these views will be presented as succinctly as possible and without comment as to the criticisms that could and have been raised against them. Suffice it to say that, in this more or less chronological presentation of major philosophical perspectives, later views are largely developed in contrast to the ones that came before. It is also hoped that this approach, in addition to clarifying Torrance's understanding of scientific theory, will pique interest in the larger issues within philosophy of science among theologians and Torrance scholars by giving something of an introduction to a field with which they may not have much experience.

In the overwhelming majority of cases, Torrance did not engage substantively with these philosophers. As such, this paper will be more concerned with comparing and contrasting Torrance's position with those surveyed in the first half of the paper rather than attempting to explain how Torrance developed his views in conversation with them, for he did not do so. This paper is envisaged as being in the same spirit as Torrance's own essay comparing and contrasting the philosophy of Michael Polanyi with other important thinkers.[2]

Positivism

One of the major movements within philosophy of science in the early twentieth century, and one that Torrance responded to, is positivism. Ernst Mach was one of the primary influences on what became the positivism of the Vienna Circle. Mach championed a robust empiricism that aimed for scientific theory to go only as far as experience went and no further. Michael Polanyi summarizes the function of scientific theory in Mach's thought: "Scientific theory, according

1 Some will notice the conspicuous absence of thinkers like Albert Einstein and Michael Polanyi from this essay. Their omission is intentional. This is not because their work is unimportant and certainly not because they were not significantly influential for Torrance, but because the role of their contributions is often overshadowed by others in philosophy of science. Additionally, as Torrance engaged in more explicit dialogue with such thinkers, they are more widely known by Torrance commentators.

2 T. F. Torrance, "The Place of Michael Polanyi in Modern Philosophy of Science," in *Transformation and Convergence in the Frame of Knowledge* (Grand Rapids, MI: Eerdmans, 1984), 107-173. A robust constructive account of Torrance's philosophy of science is beyond the scope of this paper. For such an account, see Travis M. Stevick, *Encountering Reality: T. F. Torrance on Truth and Human Understanding* (Minneapolis, Fortress Press, 2016).

to Mach, is merely a convenient summary of experience. Its purpose is to save time and trouble in recording observations. It is the most economical adaptation of thought to facts, and just as external to the facts as a map, a timetable, or a telephone directory; indeed, this conception of scientific theory would include a timetable or a telephone directory among scientific theories."[3]

Later, logical positivists like A. J. Ayer would crystallize this kind of understanding of scientific theory in the verification criterion of meaning. "The criterion which we use to test the genuineness of apparent statements of fact is the criterion of verifiability. We say that a sentence is factually significant to any given person, if, and only if, he knows how to verify the proposition which it purports to express — that is, if he knows what observations would lead him, under certain conditions, to accept the proposition as being true, or reject it as being false."[4] The key characteristic of the function of theory in positivist philosophy of science is the attempt to eliminate metaphysical considerations from scientific theory.

In this respect, positivism is seeking to remain in continuity with the scientific standards claimed by Isaac Newton. Newton famously attempted a similar elimination of non-empirical elements from scientific theory. In particular, Newton claimed that "Whatever is not deduced from phenomena, is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction."[5]

Karl Popper

There is perhaps no greater single influence on the popular understanding of science in today's world than Karl Popper. Versions of his philosophy of "critical rationalism" can be found, even in recent popular debates in science, such as that between Bill Nye and Ken Ham. The crucial difference between Popper and the positivists is that, while the positivists sought a criterion for whether a statement

3 Michael Polanyi, *Personal Knowledge: Towards a Post–Critical Philosophy* (Chicago: University of Chicago Press, 1958), 9.

4 Alfred Jules Ayer, *Language, Truth and Logic* (London: Victor Gollancz, 1964), 35.

5 Torrance, *Transformation and Convergence*, 16–18. Torrance cites this as coming from Newton, *Principia,* 575 [*The Principia: Mathematical Principles of Natural Philosophy*, tran. I. Bernard Cohen and Anne Whitman (Berkeley, CA: University of California Press, 1999), 943] and *Opticks: Or a Treatise of the Reflections, Refractions, Inflections & Colours of Light*, 4th ed. (London: G. Bell & Sons, 1931), 369.

or theory was *meaningful*, for Popper, the question was not over a statement's or theory's *meaningfulness*, but over its status as either *science* or *pseudo-science*.[6]

Popper argues that what distinguishes science from all forms of pseudo-science is that, unlike pseudo-science, science is inherently *falsifiable*. A scientific theory cannot be compatible with every conceivable piece of evidence.[7] It must be incompatible with at least *some* conceivable evidence if it is to advance our knowledge. A scientific theory could be supported by a million observations, but that is no guarantee that the million-and-first observation will not show that what seemed to be a law of nature was really no more than frequently conjoined phenomena. For Popper, a scientific theory must declare *some* observations to be impossible. In this case, it is fairly simple to falsify the theory; one needs only to produce an observation of an occurrence that the theory claimed to be impossible.

> Assume that we have deliberately made it our task to live in this unknown world of ours; to adjust ourselves to it as well as we can; to take advantage of the opportunities we can find in it; and to explain it, *if* possible (we need not assume that it is), and as far as possible, with the help of laws and explanatory theories. *If we have made this our task, then there is no more rational procedure than the method of trial and error — of conjecture and refutation:* of boldly proposing theories; of trying our best to show that these theories are erroneous; and of accepting them tentatively if our critical efforts are unsuccessful.[8]

Thomas Kuhn

In 1962, Thomas S. Kuhn published his landmark text, *The Structure of Scientific Revolutions.* In this work, Kuhn suggests that no scientific theories are perfect representations of the world as it really is but are always beset, to a greater or lesser degree, by various recalcitrant data which do not easily or naturally fit into the view of the world set forward by the theory. In light of this claim, Kuhn distinguishes between two kinds or modes of scientific activity. One of these is called "revolutionary science," in which scientists behave more or less (though not exactly) like Popper argues they should,[9] treating such

6 Karl R. Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge*, Revised 4th ed. (London: Butler & Tanner, 1976), 33-37.

7 Popper, *Conjectures and Refutations*, 36.

8 Popper, *Conjectures and Refutations*, 51, author's emphasis.

9 Thomas S. Kuhn, "Logic of Discovery Or Psychology of Research?" In *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London, 1965*, Volume 4, ed. Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970), 1.

data as a refutation of the theory and seeking a better alternative to replace it. The other kind or mode of scientific activity is what he calls "normal science," where such data is seen as merely anomalous and more or less ignored in the hopes that either subsequent research will show that the data was wrong in the first place, or else that there will arise a way to assimilate it into the reigning theoretical framework.[10] In actual practice, anomalous data are not seen as being a real *danger* to the theory until trust in the theory has been eroded for one reason or another (sometimes due to the accumulation of anomalies).[11]

While Kuhn does not wish to deny the reality and importance of revolutionary science, his book arises out of the conviction that *most* scientists spend *most* of their time within the context of normal science, where they engage in "research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice."[12] Most of what we call science is done within the context of a theoretical framework that has two characteristics. First, when the foundational theoretical work was advanced, it "was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity." Second, "it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve."[13] In this kind of scenario, Kuhn says that scientists are operating with a "paradigm."

Because scientific theory functions, in this kind of context, as an overarching foundation on which scientific research is built and a framework in which scientific achievements are fitted, the paradigm within which scientists work not only partly determine what problems scientists will find both interesting and likely to be solvable and what kinds of explanations scientists will find plausible and compelling, but it will also begin to shape the *experience* of the scientist so that they begin to see the *world* in terms of the paradigm.[14]

10 Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 3rd ed. (Chicago: University of Chicago Press, 1996), 82.

11 For an example of what it looks like for someone gradually losing confidence in a paradigm, see Kuhn, *The Structure of Scientific Revolutions*, 62-64.

12 Ibid., 10.

13 Ibid.

14 Ibid., 110-135. Some may notice a certain similarity between Kuhn's work and that of Michael Polanyi. Kuhn notes Polanyi's influence on his writing in his book. "Michael Polanyi has brilliantly developed a very similar theme, arguing that much of the scientist's success depends upon 'tacit knowledge,' i.e., upon knowledge that is acquired through practice that cannot be articulated explicitly." Ibid., 44n. Torrance notes this in *Transformation and Convergence*, 260n.

This understanding of the function of scientific theory may seem counter-intuitive, and was indeed seen as such by some, including Popper,[15] Kuhn gives some examples of how different paradigms might not only *explain* the same phenomenon in different ways but also *experience* the phenomenon differently. One example is that of what we would now call pendular movement, such as we might see in a clock. This kind of movement was not always experienced this way. According to an Aristotelian interpretation, a weight suspended at the end of a string behaves the way it does because the weight wants to find its "natural" state of rest as close to earth as possible. As such, the function of the string is to constrain the fall. Kuhn argues that these two ways of interpreting the same phenomenon imply that people who inhabit these different paradigms actually *experience* things differently.[16]

This has profound implications for those who would claim that science is to be believed and privileged over other ways of gaining knowledge because it generates theoretical representations that are either true or approximately true (we will return to this idea later). If Kuhn's account of the function of scientific theory is correct, then we cannot be assured that *any* of our theories is either true or approximately true because successive paradigms are not only different from one another, but we may not even be able to adequately *compare* them to one another. To make the implications of Kuhn's philosophy as clear as possible, he will be quoted at length.

> Let us, therefore, now take it for granted that the differences between successive paradigms are both necessary and irreconcilable. Can we then say more explicitly what sorts of differences these are? The most apparent type has already been illustrated repeatedly. Successive paradigms tell us different things about the population of the universe and about that population's behavior. They differ, that is, about such questions as the existence of subatomic particles, the materiality of light, and the conservation of heat or energy. These are the substantive differences between successive paradigms, and they require no further illustration. But paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them. They are the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely "unscientific."

15 Karl R. Popper, "Normal Science and its Dangers" in *Criticism and the Growth of Knowledge*, 51-58.

16 Kuhn, *The Structure of Scientific Revolutions*, 118-121.

> Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant achievement. And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.[17]

That is to say, after a science proceeds through a revolution, it is not usually the case that the new paradigm is better at answering all the questions that the old paradigm could answer and a few more;[18] rather it is often the case that the new paradigm will simply declare some of the issues covered by the old paradigm as no longer part of the domain of that science.

This idea, that subsequent paradigms are frequently incommensurable with one another, should not be taken too strongly, especially when dealing with their practical implications. It is sometimes the case that a replaced paradigm can retain its usefulness as a "limiting case" of the new one.[19] For example, Newtonian physics is not strictly translatable into Einsteinian physics. However, for many cases, including essentially everything in our daily lives, Newtonian physics gives us results that are indistinguishable from the Einsteinian results in practice. With appropriate restrictions, "replaced" theories and paradigms can sometimes function as special cases of those that replace them.[20]

It is difficult to overstate Kuhn's influence on subsequent philosophy of science. While his views have been critiqued in various ways, they remain perennially significant and those who work in the field are not free to ignore them. They must either accept them or else respond to them.

Imre Lakatos

Imre Lakatos was a passionate defender of Karl Popper's philosophy of falsificationism. He believed that Popper was basically right, but he was troubled by some elements of Kuhn's philosophy that he felt were warranted. There were some elements of Kuhn's paradigm theory that seemed to be sound and needed to be accepted, but there were others that he found distasteful.

17 Ibid., 103.

18 Contra Richard Feynman, *The Character of Physical Law* (London: Cox and Wyman, 1965), 165.

19 For more on "limiting cases," see Stevick, *Encountering Reality*, 184-187.

20 This is not the case for *every* replaced theory. For example, the Phlogiston theory of combustion is not a special case of the oxygen theory.

Rather than simply reject Kuhn's perspective, Lakatos sought to explain why Popper's philosophy need not deny what he felt were Kuhn's key advances.[21]

Perhaps nothing bothered Lakatos more than the fact that Kuhn not only failed to articulate the conditions under which a scientific theory should be abandoned, but claimed that it is actually impossible to articulate such conditions. For Lakatos, the difference between Popper and Kuhn could not be greater: "For Popper scientific change is rational or at least rationally reconstructible and falls in the realm of the *logic of discovery.* For Kuhn scientific change — from one 'paradigm' to another — is a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the *(social) psychology of discovery*. Scientific change is a kind of religious change."[22]

As such, Lakatos set out to defend a kind of Popperian "critical rationalism" that did not fall victim to Kuhn's historical critique. In essence,[23] Lakatos proposed that the ideal, usually thought of as Popper's own view where a scientific theory can be refuted by a single observation, is intuitively false.[24] Part of this is because "some scientific theories are normally interpreted as containing a *ceteris paribus* clause: in such cases it is always a specific theory *together* with this clause which may be refuted."[25] Lakatos provides an example: "'All swans are white,' if true, would be a mere curiosity unless it asserted that swanness *causes* whiteness. But then a black swan would not refute this proposition, since it may only indicate *other causes* operating simultaneously. Thus 'all swans are white' is either an oddity and easily disprovable or a scientific proposition with a *ceteris paribus* clause and therefore undisprovable."[26]

Lakatos disapproves of both Kuhn's position, which he believes turns scientific change into nothing more than "religious change,"[27] as well as what he regards as a naïve reading of Popper. He contrasts his more sophisticated falsificationism with a more Kuhnian perspective.

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21 In fact, Lakatos takes the somewhat questionable approach of presenting his perspective as if it actually *was* Popper's views, if not distorted by caricature. Most philosophers would consider Lakatos' account of scientific theory as going beyond Popper, even if Lakatos attempted to keep continuity with Popper's philosophy.

22 Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the Growth of Knowledge*, 93.

23 Those who wish more detail can find it in Lakatos' original exposition, "Falsification and the Methodology," 91-196.

24 Ibid., 103.

25 Ibid., 101.

26 Ibid., 102.

27 Ibid., 93.

> The Duhemian *conservative conventionalist* (or "methodological justificationist," if you wish) makes unfalsifiable by *fiat* some (spatio-temporally) universal theories, which are distinguished by their explanatory power, simplicity, or beauty. Our Popperian *revolutionary conventionalist* (or "methodological falsificationist") makes unfalsifiable by *fiat* some (spatial-temporally) singular statements which are distinguishable by the fact that there exists at the time a "relevant technique" such that "anyone who has learned it" will be able to *decide* that the statement is "acceptable."[28]

In practice, Lakatos suggests that proper rational activity consists in pursuing a "research program" in which we hold on to a "hard core" of theoretical convictions that are surrounded by what he calls a "protective belt of auxiliary hypotheses."[29] In doing this, Lakatos hopes to abandon the idea that one piece of seemingly contradictory evidence must force an abandonment of an otherwise fruitful theory without tumbling into a view that claims that empirical considerations are not seen as decisive for theory choice.

Lakatos' position, however, brings with it several implications that may be undesirable. It means that we can no longer decide whether it is rational to accept a particular theoretical framework in any given moment. The rationality or irrationality of accepting a particular research program depends upon whether it is successful or unsuccessful, which Lakatos ties with whether it leads to a progressive or a degenerating problemshift, respectively.[30] However, Lakatos also points out that we are frequently unable to decide whether a problemshift is either progressive or degenerating until many years have gone by.[31] By his own admission, he believes that the goal of "instant rationality," where we can be assured of the rationality of our behavior at any moment, is a "utopia," and that all epistemological theories which attempt to secure it for us ultimately fail.[32]

This admission, which appears after a lengthy analysis of historical case studies, raises the question as to whether Lakatos, by his own assessment, has succeeded in his defense of key elements of Popper's philosophy. If one cannot determine at any point in time whether they are behaving rationally and rationality can only be assessed after the fact, could one not say that the best one can hope for is to be deemed rational by subsequent generations of

28 Ibid., 106.

29 Ibid., 132-137.

30 Ibid., 133.

31 Ibid., 173-174.

32 Ibid., 174.

scientists and philosophers? While this does not tie the philosophical notion of "truth" to "[changing] consensus," which Lakatos clearly dislikes,[33] it could be seen as a significant critique of a crucial aspect of Popper's thought that was untouched by Kuhn's work. For Popper, we may not know for sure that we have a true theory, but we can know for sure when we should abandon a theory. For Lakatos, this decision might not be clear until decades or centuries later.

Regardless of whether any individual finds Lakatos' work convincing, anyone interested in the interaction between theology and the philosophy of science should be aware of his work as it has become rather influential among theologians and theologically-minded Christian philosophers.[34] Despite this influence, it must also be noted that Lakatos has by no means said the final word on the function of scientific theory.

Paul Feyerabend

Lakatos had a philosophical foil during his life in the person of Paul K. Feyerabend. These two men disagreed profoundly but seemed to respect one another and enjoy one another's company.[35] If Lakatos was searching for rules of rationality and sought the ideal that scientific change was either rational or else rationally reconstructable,[36] Feyerabend was skeptical of any kind of rule that is seen to govern *a priori* how science may or may not proceed.

> It is clear, then, that the idea of a fixed method, or of a fixed theory of rationality, rests on too naive a view of man and his social surroundings. To those who look at the rich material provided by history, and who are not intent on impoverishing it in order to please their lower instincts, their craving for intellectual security in the form of clarity, precision, "objectivity," "truth," it will become clear that there is only *one* principle that can be defended under *all* circumstances and in *all* stages of human development. It is the principle: *anything goes*.[37]

Feyerabend has the reputation of being a radical relativist but, as this quotation shows, he was not trying to say that *anything* counts as science but that, if we

33 Ibid., 92.

34 Perhaps most influentially in Nancey Murphy, *Theology in the Age of Scientific Reasoning* (Ithaca: Cornell University Press, 1990), and those following her, such as J. Wentzel Van Huyssteen, *Essays in Postfoundationalist Theology* (Grand Rapids: William B. Eerdmans Publishing Company, 1997).

35 Paul K. Feyerabend, *Against Method*, 3rd ed. (London: Verso, 1993), vii.

36 Lakatos, "Falsification," 93.

37 Feyerabend, *Against Method*, 19.

feel we must follow a rule rigidly and at all times, the only rule we can follow is that anything goes, or at least anything *can* go.

While Feyerabend is not known as providing a fundamentally new way of conceiving scientific theory, as his approach was mostly critical rather than constructive, he remains important for the purposes of this essay for a few reasons. First, he represents a crucial voice that can remind us to look for the limits of our theoretical constructions. Second, he represents a decidedly non-Lakatosian perspective that, even if not accepted, needs to be noted within the theology-science interaction. Third, while Torrance's references to Feyerabend are almost non-existent,[38] there are certain areas of resonance between the two men that have inspired a monograph and deserve further engagement.[39]

Bas Van Fraassen

One of the more influential understandings of the function of scientific theory to emerge in the later years of Torrance's career is the position known as "constructive empiricism" advocated by Bas C. Van Fraassen. Constructive empiricism is, in some ways, a further development of one of the key ideas of positivism, that experience is the only relevant criterion for whether we should accept a theory or not. Specifically, Van Fraassen's understanding is that "[s]cience aims to give us theories which are empirically adequate: and acceptance of a theory involves as belief only that it is empirically adequate."[40]

In spite of a family resemblance, there are some crucial differences between Van Fraassen's views and that of the positivists. Van Fraassen has no problems with accepting scientific theories that go beyond experience, only that we do not *believe* anything other than the theory's empirical adequacy. Indeed, Van Fraassen states, "I use the adjective 'constructive' to indicate my view that

38 The only explicit reference to Feyerabend of which I am aware is in an audio recording of a Q&A session after a lecture when Torrance was asked to reflect on Feyerabend. His response is rather critical and seems to reflect more an awareness of Feyerabend's reputation rather than a robust engagement with his work. https://www.gci.org/_lib/playaudio.php?program=MiscAud/TorranceGrammar2QA&title=Grammar+and+Ground (Accessed November 27th, 2017), 12:40-13:20.

39 See David Munchin, *Is Theology a Science?: The Nature of the Scientific Enterprise in the Scientific Theology of Thomas Forsyth Torrance and the Anarchic Epistemology of Paul Feyerabend*. Studies in Systematic Theology, vol. 7, ed. S. V. D. Bevans and Miikka Ruokanen (Leiden: Brill, 2011), and David Munchin, "'Is Theology a Science?' Paul Feyerabend's Anarchic Epistemology as Challenge Test to T. F. Torrance's Scientific Theology," *Scottish Journal of Theology* 64 (2011): 449.

40 Bas C. Van Fraassen, *The Scientific Image* (Oxford: Oxford University Press, 1980), 12.

scientific activity is one of construction rather than discovery: construction of models that must be adequate to the phenomena, and not discovery of truth concerning the unobservable."[41]

Van Fraassen's views also have resonances with Popper because of his stress on empirical adequacy. As stubborn phenomena count against the "truth" of a Popperian conjecture, so they count against the "empirical adequacy" of a Van Fraassenite theory. There is, however, the stronger notion that we are committed to our theories: "Acceptance involves not only belief but a certain commitment. Even for those of us who are not working scientists, the acceptance involves a commitment to confront any future phenomena by means of the conceptual resources of this theory. It determines the terms in which we shall seek explanations."[42] For Van Fraassen, the importance of scientific theory is not so much in providing knowledge of the structure of the world, but in aiding the scientist in designing experiments.[43] As such, Van Fraassen is a committed anti-realist when it comes to interpreting scientific theories. However, he remains much less hostile to extra-empirical theoretical construction than the positivists.

Scientific Realism

One philosophical tradition that would seem to be a natural ally with Torrance's epistemological concerns is that of "scientific realism." Torrance often spoke of himself as a realist and valued realism to a high degree. However, if someone comes to the term "scientific realism" through Torrance's writings, they might get the impression that he presents something like the consensus view of what philosophers mean when they defend realism within the philosophy of science. This is not the case.

There are so many views that fall under the name "scientific realism"[44] that to attempt any unified discussion under this heading is bound to lead to confusion. While James Clerk Maxwell and Albert Einstein could truly be called "realists," this paper will focus on a particular tradition within scientific realism that, it is hoped, will illuminate ways in which Torrance's views are different from

41 Ibid., 5.

42 Ibid., 12.

43 Ibid., 73.

44 It does not do much to clarify things if we restrict ourselves to views calling themselves "critical realism," as there are many such views and they are by no means identical. For a discussion of the different ways the term has been used, see Andreas Losch, "On the Origins of Critical Realism," *Theology and Science* 7, no.1 (2009): 85–106.

the mainstream of scientific realism within philosophy of science in the latter twentieth century.

1962 was a watershed year in philosophy of science. It saw the publication of Kuhn's massively influential *Structure of Scientific Revolutions*. There was a second significantly important philosophical work published that same year, Grover Maxwell's "The Ontological Status of Theoretical Entities," that set out to establish a realistic interpretation of scientific theories over and against the characteristic positivist practice of treating scientific theories as "convenient fictions" or "calculating devices" in which "talk about [theoretical] entities is translatable without remainder into talk about sense contents or everyday physical objects."[45] Such views, to Maxwell, were so manifestly false that he hoped that his paper would be nothing more than a "demolition of straw men." The paper is a grand manifesto about the failure of positivistic philosophy of science.

Since the publishing of Maxwell's paper, many scientific realists have written to defend the reality of our theoretical entities. One of the more comprehensive defenses of this tradition of scientific realism comes from Stathis Psillos whose monograph, *Scientific Realism: How Science Tracks Truth*,[46] takes up many of the main arguments for and against realism. Psillos' aim is to defend "the view that mature and genuinely successful scientific theories should be accepted as nearly true" which, to him, is "an intuitively compelling philosophical claim."[47]

In defending this claim, Psillos divides his book into four parts dealing with, respectively, the failure of strict empiricism, challenges to scientific realism, criticisms of alternatives to realism, and an attempt to provide a helpful articulation of the kinds of "tools" the realist needs to be able to sustain their position, such as the concept of verisimilitude and the reference of our theoretical terms. To express this structure in other terms, Psillos' approach seems to be primarily that of putting forth a philosophical position and then defending it more by attempting to refute its rivals than by constructing arguments for its acceptance or plausibility. This is not necessarily to its detriment. It does, however, reveal that Psillos believes the burden of proof should lie on those who would disagree with him rather than himself.

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45 Grover Maxwell, "The Ontological Status of Theoretical Entities," in *Philosophy of Science: The Central Issues*, ed. Martin Curd, J. A. Cover (New York: W. W. Norton & Company, 1998), 1052.

46 Stathis Psillos, *Scientific Realism: How Science Tracks Truth*, Philosophical Issues in Science, ed. W. H. Newton-Smith (New York: Routledge, 1999).

47 Ibid., xvii.

Much could be said on the question as to whether to seek "approximate truth" is the proper goal of our scientific theories,[48] but Psillos represents something of a consensus view of the mainstream of the scientific realist tradition, though it should be noted that realist philosophers can be so divided as to the specifics of their views that philosopher Jarrett Leplin says they make a majority perspective appear as the minority.[49] What various realist perspectives tend to have in common is a stress on the reliability of our scientific theories to generate statements that are true. Science is a truth- (or approximate truth-) generating enterprise and should be relied upon as such. Much stress is laid on the fact that realism "works," in that scientists who rely upon a realistic interpretation of their theories tend to be able to marshal that knowledge to the end of actual technological achievements that, it is argued, would not be possible if scientific theories were merely conventions.[50] While Torrance describes himself as a realist and shares certain interests with this mainstream of scientific realism, there are significant differences between them.

T. F. Torrance

We have surveyed several important understandings of the function of scientific theory throughout the twentieth century. For the positivists, scientific theory is the cataloguing of experience for organization and analysis. For Popper, scientific theories are bold, but fallible, conjectures we make about the nature of the world. For Kuhn, a scientific theory (in his sense of "paradigm") is a coherent story we tell about the world that has explanatory power but that makes no claim to final authority. For Lakatos, theories are tools with a "hard core" of theoretical convictions with a "protective belt" of "auxiliary hypotheses," that aim to produce "novel facts." For the realists, theory aims at giving us an account of the world that is at least "approximately true." Some of them overlap to one degree or another, while others represent more or less radical breaks with the views that have come before them.

Thomas F. Torrance, as a theologian interested in the theory and practice of science, has his own nuanced perspective on how science operates and on the function of scientific theory. Torrance's views cannot be completely separated from the views already surveyed. His views will bear a certain family relation to

48 See Stevick, *Encountering Reality*, 112-114.

49 Jarrett Leplin, "Introduction," in *Scientific Realism,* ed. Jarrett Leplin (Berkeley: University of California Press, 1984), 1.

50 See Norris, *Against Relativism: Philosophy of Science, Deconstruction, and Critical Theory* (Oxford: Blackwell, 1997), 248-264.

these others, especially in their implications. These similarities do not, however, diminish the significance of Torrance's perspective.

Science, whether natural or theological, is concerned with knowledge of reality and, for Torrance, we know something authentically only when we know it in accordance with its own nature (*kata physin*). This conviction is so central to Torrance's theological and epistemological concerns that it has been called the "fundamental axiom of Torrance's theology."[51] This seemingly innocuous and even obvious axiom, when unpacked, has some profound implications for the function of scientific theory.

Perhaps the most far reaching implication for the purposes of this paper is that this conviction means that reality itself must always take precedence over our theoretical representations of it, no matter how good or helpful they may be.[52] This is perhaps most clear in Torrance's writings when he uses Anselm as a dialogue partner to articulate a three-fold meaning of "truth."[53] First, there is truth when a sentence makes grammatical sense, a usage that could be called the syntactical truth of the sentence. Almost no one uses "truth" in this sense, but Anselm and Torrance want to include it under this term.

The second use of "truth" is the way almost everyone throughout history has used it, to describe a sentence that not only makes syntactical sense but actually refers faithfully to a state of affairs beyond itself. In this case, "truth" is something that characterizes our *statements* in their *relationship* to reality. Statements are more or less true to the degree that they more or less faithfully represent what is the case. This is the kind of truth that scientific realists hope to achieve in scientific theory. The perfect theory is one that tells "a literally true story of what the world is like."[54] Realists are quick to point out that, while it is their goal, they do not necessarily believe they have achieved such a literally

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51 Elmer M. Colyer, *The Nature of Doctrine in T. F. Torrance's Theology* (Eugene, OR: Wipf and Stock, 2001), 15.

52 T. F. Torrance, "Theological Realism," In *The Philosophical Frontiers of Christian Theology: Essays Presented to D. M. MacKinnon*, (Cambridge: Cambridge University Press, 1982), 179.

53 Torrance's major discussions can be found in his *Reality and Evangelical Theology* (Philadelphia: Westminster, 1982), 126–37; *Reality and Scientific Theology,* Revised 2002 (Edinburgh: Scottish Academic, 1981), 143–47; "The Place of Word and Truth in Theological Inquiry According to St. Anselm," in *Studia Medievalia Et Mariologica*, P. Carolo Balic OFM Septvagesium Explendi Annum Dicta, ed. P. Zavalloni (Rome: Antonianum, 1971), 142–47; "Ethical Implications of Anselm's De Veritate." *Theologische Zeitschrift* 24, no. 5 (1968): 309–13.

54 Van Fraassen, *The Scientific Image*, 8.

true story, and that science, in the meantime, "aims at fruitful metaphor and at ever more detailed structure."[55]

If the first meaning of "truth" is the truth of a statement (in the syntactical sense), and if the second meaning of "truth" is the truth of a statement's signifying something beyond itself, the third meaning of "truth" is the truth of "being." Torrance, following his reading of Anselm, uses "truth" in a way to speak of something being what it is and not something else. While this usage of truth is non-standard (as most philosophers use it in the second sense above and may find this third usage discomforting), it highlights the crucial element of his understanding of the function of scientific theory.[56] Our theories can be true to the degree to which they adequately bear witness to the truth of reality itself.[57] However, just as we may never conflate our statements with that to which they refer, so we may never conflate reality with our knowledge of it.

This highest level of truth, in Anselm, is explicitly tied to the being of God, upon which all other truth depends. This helps to illuminate Torrance's distinctly Christian starting point in his reflections on truth. For Torrance, the marginalization of the truth of statement by the truth of being is not merely one convention among others, but one that is demanded by the gospel itself.[58] All theology is an attempt to bear witness, through our statements, to the living Word of God.

This approach, arising out of distinctly Christian convictions, has implications that overflow into philosophy of science. Very few, if any, of these implications, when taken individually, are unique or without precedent in the history of the philosophy of science. Taken as a whole, however, Torrance's position is noticeably different than each of the philosophers surveyed above. Torrance develops his understanding of the function of scientific theory as an overflow of his Christian convictions. This stands in sharp contrast to the practice of the logical positivists, who began with our experience of nature; of Popper, who responded to the weakness of such a view; of Kuhn, who began with the history of science; and of

55 Ernan McMullin, "A Case for Scientific Realism," in *Scientific Realism*, ed. Jarrett Leplin, 35.

56 For a considerably expanded discussion of Torrance's understanding of truth, see Stevick, *Encountering Reality*, 99-145.

57 It is interesting that, while Torrance almost never speaks of our theories "bearing witness" to the truth of reality, it is essentially the same relation as between our biblical-theological statements and the realities of the gospel. For the closest Torrance comes to using "bearing witness" in this way, see *Theological Science* (Oxford: Oxford University Press, 1969), 331-332.

58 Ibid., 134.

the scientific realists who, since Maxwell's paper, seem to begin with something like a scientific version of apologetics.

It is at this point that Torrance's position becomes noticeably different from the mainstream of scientific realism as discussed above. It was mentioned that, even when scientific realists reject the idea that our current theories provide a "literally true story of what the world is like," that is the ultimate goal of our theories. It is hoped that eventually our theories will indeed function like that, providing something of a one-to-one correspondence with reality.

For Torrance, by contrast, the function of scientific theories is to facilitate our attempt to "make contact" with reality in order to gain kataphysic knowledge of it.[59] As we come into contact with reality through our theories, we allow the inherent rationality of reality to call our theoretical formulations into question and force their revision. In this way, Torrance's views find a parallel in those of Karl Popper. However, Torrance disagrees with Popper in a few ways. While Torrance acknowledges that even our best theoretical formulations *risk* falsification, they do not *seek* falsification.[60] Rather, they seek to disclose reality to one degree or another, even if it should turn out that they are inadequate to the task.

Torrance calls attention to this fundamental function of scientific theories by calling them "disclosure models."[61] Our theories are models of reality. However, they do not seek to be "picturing models," where the assumption is that there is, or should be, a one-to-one relationship between the model and reality. Rather, they seek to become transparent media through which we discern reality as it really is.[62] In this way, there is a resonance between Torrance's and Kuhn's

59 Torrance takes this term from Polanyi: "One may say, indeed, quite generally, that a theory which we acclaim as rational in itself is thereby accredited with prophetic powers. We accept it in the hope of making contact with reality; so that, being really true, our theory may yet show forth its truth through future centuries in ways undreamed of by its authors." *Personal Knowledge*, 5.

60 Torrance, *Transformation and Convergence*, 121.

61 Torrance's key discussions of disclosure models can be found in his *Reality and Evangelical Theology*, 49–51; *Reality and Scientific Theology*, 85–86; *The Ground and Grammar of Theology* (Charlottesville, VA: University Press of Virginia, 1981), 124–27. See David Munchin, *Is Theology a Science?*, 227–233, for a discussion on "fluid axioms" that are deeply related to disclosure models. Indeed, they are largely just a different angle on the same topic.

62 For the language of "transparent medium," see T. F. Torrance, *Divine Meaning: Studies in Patristic Hermeneutics* (Edinburgh: T&T Clark, 1995), 319; *God and Rationality* (Oxford: Oxford University Press, 1971), 120; *Reality and Evangelical Theology*, 96-97, 117; *Ground and Grammar*, 125-126; *Theological Science*, 28, 39-40, 239-240, 245-246, 298; *Theology in Reconstruction* (Grand Rapids, MI: Eerdmans, 1965), 57; *Transformation and Convergence*, 89-90.

views. Disclosure models need to be coherent within themselves and, while they seek to be a faithful representation of reality, they need not tell a literally true story of what the world is like. Indeed, should use of the disclosure model have the result of new insight and understanding that results in its being marginalized or discarded, it will be because it has succeeded, not failed, in doing its job in facilitating contact with reality which was able to stand in judgment over it.[63]

Torrance uses a handful of different metaphors to describe the way theory functions. Theory can be seen as a lens through which we discern reality, though subsequent investigation may reveal that any one of our "lenses" may have distorted reality in one way or another.[64] In his early work, Torrance used the idea of "analogue" to describe the function of our theories.[65] One helpful way of understanding the kind of investigation that, according to Torrance, helped to facilitate the development of modern science is the change in legal questioning from *quaestio* to *interrogatio*.[66] His account of this shift is worth quoting at length:

> [Lorenzo Valla] wanted something more than the kind of question that had been traditionally asked in the West after Boethius, which was directed at untying a knot in some tangled piece of knowledge that we already have. In the mediaeval mode, this proceeded by posing problem questions, drawing distinctions, and by a logical process of argumentation for and against, straightening out the lines of thought from the premises to the conclusions: but all that this seemed to succeed in doing was to clarify knowledge that we already have. What Valla wanted was a mode of inquiry in which questions yield results that are entirely new, giving rise to knowledge that we cannot derive by an inferential process from what we already know. He found that kind of question in the works of the Latin Stoic lawyers and educators like Cicero and Quintilian: that is, for example, the kind of question employed in a court of law where documents,

63 Note that this appraisal of the replaced theory is different than Popper's, whose philosophy claims that theories are replaced when they fail, rather than when they succeed.

64 For "lenses" language, see Torrance, *Incarnation*, 233; *Reality and Evangelical Theology*, 49-51, 117-118; *Reality and Scientific Theology*, 54-55, 147; *The Christian Frame of Mind* (Colorado Springs, CO: Helmers and Howard, 1989), 149; *Ground and Grammar*, 125-126; *The Mediation of Christ,* Revised ed. (Colorado Springs, CO: Helmers and Howard, 1992), 20; *Transformation and Convergence*, 89-90, 273-274.

65 With the exception of the quote from *Theological Science* below, it seems that Torrance's use of "analogue" was more or less restricted to his *Theology in Reconstruction*.

66 For the relevant passages in Torrance, see *God and Rationality*, 34; *Juridical Law and Physical Law,* 2nd ed. (Eugene, CO: Wipf and Stock, 1997), 37; *Transformation and Convergence*, 267–268; *The Hermeneutics of John Calvin* (Edinburgh: Scottish Academic, 1988), 111–112.

> witnesses, states of affairs are interrogated directly and openly, without any prior conception of what the truth might be, so as to let the truth itself, the whole truth and nothing but the truth, come to view.... Calvin applied it to the interpretation of the Scriptures, and thus became the father of modern biblical interpretation, but Francis Bacon applied it to the interpretation of the books of nature, as well as to the books of God, and became the father of modern empirical science, not of course that he was himself a great scientist — he lacked the mathematics for that — but he conceived of the empirical method which was to become so tellingly important.[67]

Torrance describes what this kind of engagement looks like within the field of the natural sciences:

> In the process of question and answer in some field, we find imposed upon us a new and enlightening form which we judge to be an important intimation or essential clue to the reality we are investigating. We make it central and organize the other forms round it in a harmonious pattern of reference. Then we imaginatively and tentatively project that as a hypothesis and put it as a complex question to the reality we are investigating in such a way that the answer is clearly intuited, and so once again in the light of what is revealed we proceed to reconstruct it. We clarify and sharpen its focus as an act of interrogation, we simplify and unify its conceptual form, in the hope that it will become such a transparent medium for our apprehension that our thoughts will fall under the power of the logic or the interior connection in the components of reality itself. This is the theory or "mechanism," what we now call a "model," or better still an "analogue" (especially for the more concrete and less mechanical sciences), but it remains only an instrument of reference in the successive advances of our cognitive interrogation, a kinetic model or analogue that is to be "operationally defined" (in Einstein's sense), and must never be allowed to become fixed or rigid for that would suppress its intended function in discovery.[68]

Much more could be said about what exactly is implied by Torrance's notion of "disclosure models,"[69] but this summary of his position is sufficient to demonstrate both Torrance's views and where they can be situated relative to the others surveyed above. As such, much of the remainder of this paper will consist of a close reading of this quotation and its implications.

Torrance believes that the key to the development of a scientific theory is to have "imposed upon us a new and enlightening form which we judge to be

67 Torrance, *Transformation and Convergence*, 267-268.

68 Torrance, *Theological Science*, 239-240. See also *Reality and Scientific Theology*, 26–27.

69 See Stevick, *Encountering Reality*, 159-195.

an important intimation or essential clue to the reality we are investigating." Already we can see the wide gulf between Torrance and the positivists. The very first step of theory development involves going beyond a strict representation of experience and the role of personal judgment in deciding which experiences are central and which are peripheral.[70]

We then proceed by putting our incipient theory "as a complex question to the reality we are investigating in such a way that the answer is clearly intuited." So far, this looks rather a lot like Popper's procedure of conjectures and refutations. However, Torrance's perspective is more shaped by realist convictions. For Popper, a scientific theory is a bold conjecture which we hope will be true and which, ideally, we know ahead of time what new evidence would cause us to give it up and create a new theory. By contrast, Torrance suggests that we put this "question" to reality with the expectation that it will not, in fact, turn out to be entirely correct but that a theory can be *inadequate* (even *significantly*, or non-trivially inadequate) and yet not necessarily be *false*.[71] Reality might not only say "no" to our false conjectures, it may also say "kind of" to our question. When this happens (this, again, is the expected result of a Torrancean disclosure model), our next step is not to throw out the theory and begin from scratch, but to modify and adjust our model. By doing so, we hope to ask a better "question" in order to get a better, or at least more illuminating answer.

The goal of this process of posing and revising our questions to reality is the hope that over time we will be left with something that "will become such a transparent medium for our apprehension that our thoughts will fall under the power of the logic or the interior connection in the components of reality itself." This goal reveals both Torrance's profoundly realist concerns as well as differentiation from the mainstream of realist philosophy of science.

For Torrance, the goal is for us to be able to encounter without distortion reality itself and come under the compulsive authority of its own inherent rationality. This is decidedly more realist than Kuhn's position which, while stressing the importance of coherence, is relatively unconcerned with what is "really out there."[72] Torrance's view is also more realist than Van Fraassen's

70 See Polanyi on connoisseurship. *Personal Knowledge*, 54-55.

71 For Torrance's repeated claims that the inadequacy should not be seen as implying its falsehood, see *Divine Meaning*, 65; *Reality and Scientific Theology*, 89; *Theological Science*, 86; *Theology in Reconstruction*, 50, 51, 69-70, 90-91.

72 Even if this was not Kuhn's intent, there are places where he seems to be a more thoroughgoing relativist. "If I am right, then 'truth' may, like 'proof,' be a term with only intra-theoretic applications." Kuhn, "Reflections on My Critics," in Lakatos and Musgrave, *Criticism and the Growth of Knowledge*, 266.

because it seeks not merely to "save the phenomena," but to actually connect with the inherent intelligibility of reality.[73]

The goal of our scientific theories also reveals the significant difference between Torrance and other philosophers of science. Torrance is not interested in seeking theories that are to be accepted because they are "true" or "approximately true," or because they are the result of an inference to the best explanation available to us at a given moment in time. Our theories may turn out to satisfy any or all of these stereotypically "realist" concerns, but satisfying them is not the goal of Torrance's realism. The goal is not to develop theories that are characterized by the truth of *statement* but that allow us to make contact with the truth of *being*. It is because Torrance makes this not entirely uncontroversial distinction within the very notion of "truth" that his realist convictions seem out of step with much of realist philosophy of science.

According to Torrance's account of the function of scientific theories, scientists proceed with their empirico-theoretical engagement with their objects of study by adopting a flexible, inherently revisable manner of investigation that is conscious, as far as possible, of its presuppositions and is self-correcting of them. Over time, reality will continue to disclose itself to us through these models resulting in their change. Sometimes, this change will be incremental, and will look like Kuhnian "normal science." Other times, it will be dramatic, in which case it will look like Popper's program of conjectures and refutations.

Conclusion

In the light of this survey of key thinkers we can see that Torrance's understanding of the function of scientific theory has resonances with each of them, with the possible exception of the positivists. Like Popper, Torrance strongly affirms the "right" of reality to call all of our most cherished theories into question, forcing their revision into something more appropriate. Like Kuhn, Torrance believes that our engagement with reality is never theory-neutral but that our theory choice can influence the "answers" we receive from reality and

73 Bas C. Van Fraassen, "To Save the Phenomena," in *Scientific Realism*, ed. Jarrett Leplin, 250-259. It should be noted that Van Fraassen is not always as radically anti-realist as he claims. "In just the same way, I claim that the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinist) mind. For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive — the ones which *in fact* latched on to actual regularities in nature." *The Scientific Image*, 40. These "actual regularities" seem to have metaphysical implications. We also see a similar oblique reference to realist convictions in Wittgenstein. See Stevick, *Encountering Reality*, 150-157.

that we must strive to develop a framework of thought that is faithful to what we are investigating. Like Lakatos, Torrance wants to admit the important points raised by Kuhn without tumbling into relativism. Like Feyerabend, Torrance is committed to a radically *a posteriori* approach to scientific knowledge, refusing to decide before investigation how we must conduct that investigation.[74] Like Van Fraassen, Torrance rejects the idea that the *goal* of scientific theory is to achieve an entirely adequate representation of reality in our statements. Like the realists, Torrance believes that we are not free to develop merely coherent accounts of what reality *might* be like but must press on to relate our theories to how the world actually is.

There are, of course, significant differences between Torrance and each of these philosophers. For example, Torrance believes that Popper's theory "assumes that the relation between our concepts and being can be specified in a clear and determinate manner,"[75] which he rejects. Torrance's staunch realist concerns would also likely incline him to reject Kuhn's tendency toward relativism, though he never states this explicitly.

While Torrance does not do a great job explaining his own views relative to the rest of the philosophical landscape at the time, and while his own views are seldom, if ever, unique, he does have a compelling understanding of the function of theory in the natural and theological sciences. It is not clear whether Torrance developed his views in conscious engagement with the thinkers in this paper. However, for all his similarities with them at any given point, his views are distinct from all of them and function as something like a synthesis of many of their crucial insights.

Whether or not Torrance is to be followed in every aspect of his understanding of the function of theory in natural and theological science, he provides a helpful model of a theologian who is sufficiently engaged with scientific practice and philosophy of science so as to be able to provide his own account of science that is worthy to be considered alongside the greatest contemporary philosophers of science. This is a welcome example for any who may worry that theologians interested in science may participate only by appropriating the work of secular philosophers and scientists. Torrance shows us that, even in our contemporary situation, theologians may be able to suggest ways to move beyond false philosophical dichotomies.

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74 Also, the two men are agreed that there are no rules for how we must verify knowledge gained in the context of justification. See Stevick, *Encountering Reality*, 92-93.

75 Torrance, *Reality and Scientific Theology*, 49.